AN

ESSAY

ON THE

ABSORBENTS;

COMPRISING

SOME OBSERVATIONS

UPON THE RELATIVE

Pathologies and Functions

OF THE

Absorbent & Secreting Systems.

BY DANIEL PRING,

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TO

GEORGE FREER, ESQ;

MEMBER OF THE ROYAL COLLEGE OF SURGEONS, LONDON;

AND SENIOR SURGEON TO THE GENERAL HOSPITAL

NEAR BIRMINGHAM;

These Pages are inscribed,

AS AN EXPRESSION OF RESPECT, GRATITUDE,

AND ESTEEM,

FOR THE NUMEROUS OBLIGATIONS AND ADVANTAGES

CONFERRED UPON HIS CI-DEVANT PUPIL,

THE AUTHOR.

PREFACE.

THE following Essay was originally prepared for a purpose which, on its being finished, I felt convinced it was on many accounts but ill calculated to ensure. Without therefore subjecting it to a trial, I resolved, with the advice of those whose opinions I had reason to hold in no small estimation, to give it, perhaps, more than its meed of publicity in another shape.

I felt adverse to its entire suppression, not because I was ridiculous enough to indulge a high notion of its merits or utility, but because I should otherwise have had to regret the total waste of the time which was employed in its execution.

If its real claims to notice are inconsiderable, the manner of its introduction is no less devoid of form and ostentation; and its pretensions are designed only to commensurate with whatever pittance of commendation may be subsequently allowed.

To invite, in any degree, attention to a performance confessedly and unquestionably of no merit, is to be guilty of perhaps even a greater impertinence, than to deck out the poverty of thought and matter in a garb of importance which it does not deserve; and which will first elicit the notice of mankind, and afterwards most grossly disappoint the expectations which were raised only by the bulk, pomp, and tinsel of appearances.

In order therefore that I might not fall under the correction of my own remark, it may be urged that the publication of the essay was recommended me on grounds, not to have concurred in which, would have been to form an unprecedented exception to a propensity which pervades human nature, and obtains more or less in every department and occasion of life. Another motive arose out of the ambiguity in which the principles and doctrines belonging to the absorbent system are involved. These principles are in short indefinite, contradictory, and oftentimes leading to opposite modes of treatment under the direction of different individuals. It is principally with a view to elucidate these, to reconcile such among them as may disagree, and to deduce occasionally from such regards a new indication of treatment, that I have ventured to

give publicity to those lucubrations which, on other accounts, would perhaps have been limited with more propriety to the purposes of my own private amusement.

I cannot suppose but that I have been, on many particulars, too diffuse and elaborate; while others are treated with a brevity which does not quadrate with their merits and importance. I have only to plead in extenuation of these faults, that the path is almost untrodden, and that I have been able to derive but little assistance from previous inquiries and authorities.

If I were disposed to compile from such authorities as *are* to be obtained, it would be an easy matter to put together a most copious tissue of comments, disquisitions, and quotations, from a single work;* and to subjoin a host of references which should look like "confirmation strong as proof of holy writ." But as the end of such details would be to leave our science precisely where it was before, I cannot bring myself to submit to so much drudgery, without being impelled to it by any worthy or sufficient motive.

* Soemmerring de morbis vasorum absorbentium corporis humani.

It was in compliance with my original design that so many particulars were embraced; and it arose out of this supposed necessity that the regards bestowed upon each are compressed into limits and suggestions so brief and cursory.

The pathology of the absorbents, considered in all its relations, is a field of boundless extent; and to treat of every disease in which this system is liable to participate, would occupy volumes.

It is on this ground that I have limited my observations to those points which may be said to form the principal difficulties of the subject; to those points of doctrine which derive an importance from their practical relations; and from those indications of treatment to which they must necessarily give rise where the lesson of experience is scarcely formed, and where the chances of empiricism are in a great measure precluded.

Essay on the Absorbents, &c.

THERE are perhaps few subjects to which the attention of the surgeon and the physiologist is more pressingly invited, than the hitherto unexplained process and phœnomena of absorption. It merits the highest regard from the surgeon, because it is a function important in itself, and rendered still more so by the frequency of those occasions wherein it is necessary to operate upon it. There is a mystery about it, which obscures all our reasonings upon the subject, rendering them indefinite and altogether unsatisfactory. It is the emulation of the physiologist to remove those clouds and ambiguities in his science, by which its perfection is retarded, and by which too strict a limitation is prescribed to the benefits which would otherwise result from it.

Difficulty both is and ought to be the incentive to research. Our exertions may perhaps at first be productive of nothing but disappointment; the field of discovery is open to our labors; but early inquiry, where imagination is silent, must lead us only to regret the insufficiency of our faculties, to deplore that narrowness of comprehension which enables us to perceive, but denies us the attainment of our ends.

Theory, unsupported by facts or other evidence in any shape cognizable to the senses, was once the fashionable mode of philosophising. Hypothesis held the rank of induction; and fancy, but ill adapted for the seriousness of investigation, assumed those reins which reason alone is worthy to command. But the rise, progress, and decline of fashions of every description are rapid in their succession. In this instance, a motive more laudable than the mere love of novelty may be conceived to have interposed; unfortunately, however, the overthrow of one system is too frequently only the prelude or introduction to its opposite. Men do not love their own opinions by halves; and, in the violence of affection, they magnify their importance, their beauty, and every other quality which can recommend them, until they grow warm in their behalf, and an unwarrantable ardor impels them to generalize that which should be taken with much limitation.

In medicine, the rage of hypothesis had scarcely cooled before its votaries were seized with an universal mania for evidence of an indisputable kind: nothing less than a demonstration by rule and figure could possibly introduce an opinion with any semblance of respectability; and it is surprising that the lapse of a century should be suffered to glide by, before the world began to suspect the inaptitude of mathematical reasonings for the purposes to which they were applied. Opinions widely different prevailed upon the same point, and that too, although the proposers of them had arrived at their respective conclusions by a similar method of inquiry. Need we look for a more striking illustration than that which the calculations of Keil and Borelli afford? Could it be supposed

that mankind would any longer submit to be hoodwinked by a mode of investigation so palpably imperfect? Could they any longer remain blind to the folly of raising a superstructure of problematical inferences upon data, which at best were conjectural and assumed?

Thanks to the good sense of more modern times, theory and experiment go hand in hand *together*. What the one wants in solidity is supplied by the other, and the dullness of empiricism is spiritualized and adorned by the comprehensive designs of intellectual effort. But notwithstanding the improvement hinted at, we have frequently much to regret, from the insufficiency of this best mode of philosophising; and we always shall have much to regret, as long as the irregularities of result throw an ambiguity upon those ultimate principles to whose just nature our understandings cannot penetrate, but which must nevertheless serve as the basis for investigation. The evidence of experiments is often-times contradictory; and their want of success is more frequently attributable to the defective reasonings employed about them, than to any inaccuracy in the process of the experiment. But as we had not the honor of making them ourselves, we must lay the imperfections of our faculties to another account. Perhaps it is not much to be lamented, that they are no better than we find them; had they been cast in a vastly superior mould, the world of discovery had long since been travelled through, intellect had lacked employment, and we, born unfortunately in an age somewhat remote from the beginning, should have nothing else to do but to admire, and grow stupid with admiration. The glory of participating in these honors would then have been denied us, and the delight of a new idea

beaming in upon our understandings would never have been felt. Our faculties, therefore, though straightly limited, are good enough for our purposes; and though the helps of method are alike imperfect, we must nevertheless content ourselves with those which appear to be the best, happy if any thing desirable can result from materials so rude and unfinished.

A Sketch of the History of the Absorbents.

TO perfect discovery of any kind is the work of posterity and after-ages. The clew is first laid, in slight notices and obscure and casual glimpses; the conclusions which are derived from such partial observances are generally erroneous, and frequently ridiculous. But it is the business of a succeeding age to begin where the last left off; and thus the work of improvement, like an ancient patrimony, is committed for its cultivation to the care and industry of future generations. Our thanks, however, are as much due to the hands which cherished and protected the infancy of science, as to the diligence and devotion by which its growth was matured. Had Newton lived before Des Cartes, his career of discovery had perhaps ended where it has now begun; making, however, some little allowance for the different eccentricities of their genius. If one should select a subject wherein the observations would be better illustrated and exemplified than in any other, I think we should recur to the his-

tory of anatomy and physiology. The one, in a medical point of view, is useful only from the intimate alliance it claims with the other. But by the surgeon anatomy must be respected for its own sake. Although so close a connection obtains between them, as to render them worthy the appellation of sister sciences: although it would appear that an intimacy with the one must necessarily lead to an acquaintance with the other, we are compelled nevertheless to acknowledge the converse of this supposition. Anatomy and physiology, though departments of the same subject, are widely distinct; and demand, for the success of their respective investigations, a mode of inquiry altogether different.

The progress of the one is every where characterized by a demonstrative evidence—the evidence of the senses. The first step of information which is attained on the other is the result of the exercise of reason; it is a conclusion from data, which, as far as they go, must be correct, in as much as they generally admit a previous demonstration: but the conclusion itself is marked by a diversity, as mutiform as the regards which the caprice of individuals or their different habits of thinking may incline them to bestow upon the matter.

Seeing that even in the present day the developement of structures is, in many instances, inadequate to throw the faintest light upon the functions to which they subserve, we cannot be surprised that physiology should in the earlier ages have limped so lamely after the discoveries of the knife. We cannot feel surprised that a knowledge (such as it was) of the structure of the heart, and of the general arrangement of the blood-vessels,

should have preceded, by such a lapse of time, the discovery of the circulation of the blood. It is not to be wondered at that Herophilus and Erisistratus, the two anatomical fathers of the Alexandrian school, should have perceived the lymphatics of the mesentery, and have contented themselves with such brief observation, without expressing any thing like a plausible conjecture concerning their nature or their relations in the animal economy.

Anatomy, at that time, was by some prosecuted as an almost independent science. Men of sense saw the darkness and ambiguity in which physiological research was involved; accordingly they directed their labors to that end which they could with some shew of reason expect to attain.

The complete discovery of the functions of the lacteals is deservedly attributed to Asellius. It was he who first conjectured upon the subject with reason, and proved by reiterated experiments, instituted expressly for that purpose, the *use* of the lymphatics. But the jealousy of cotemporaries would not allow him to enjoy his reputation, without litigating the justness of his claims. To him, however, it is generally agreed, the honor is due.

Erisistratus cannot be said to have been altogether ignorant of the functions of the lacteals; it must be allowed that he was *imperfectly* acquainted with their physiology: but the respectability of one part of his conjectures was entirely disgraced by the ignorance displayed in another. He conceived that the vessels which he saw distended with chyle in the mesentery of

a kid, were *arteries*; but he likewise assigned them the office of carrying milk from the intestines of the animal to the *liver*, for the purposes of nutrition.

A somewhat similar idea was entertained by Herophilus, Galen, and some others; but they supposed that these vessels did not terminate in the ven. portar. but that they were distributed to the glands of the mesentery, and did not pass beyond them. All these notions were extremely crude; the thing was first made to assume a somewhat respectable form by Asellius; and it is wonderful that a circumstance, which must have been so frequently the subject of observation, should so long have escaped a more particular notice; that it should never have been understood, in its first and most simple relations, until so late a period as the year 1622.

We are informed by their discoverer, that the structure of the lacteals is similar to that of the veins; that their external surface is smooth, while they are lined internally with a membrane, which forms, at irregular distances, valvular productions. He likewise tells us, that these vessels proceed more abundantly from the jejunum than from any other intestine; that the sizes and capacities of them are various. So far he has observed with correctness; but, in proceeding further, he unfortunately lost his way, and followed a track which led him very wide of his true destination. He asserts that the lacteals terminate part in the cells of a pancreatic gland, which he places in the mesentery, and the rest in the liver, venæ cavæ, and venæ portarum.

I shall pursue no further the inquiries of Asellius; we are indebted to him for some correct delineations, but he has foiled the respectability of one part of his statement by the error and falsehoods which he has introduced into another.

Veslingius wrote something about the lacteals, and even made drawings of them, such as he conceived them to exist in the human subject. But his description is borrowed as far as it is correct, and he has been much more prolific in the offsprings of imagination than in the productions of the knife.

Harvey supposed that the chyle was absorbed from the intestines by the veins, and of course disputed the conclusions of Asellius. But these conclusions were confirmed by the labors of other anatomists,—by Severinus, Rolfinckius, Hermius, and Hildanus, and every successive light upon the subject reflected additional proof of their accuracy.

About the year 1649, Pecquet, in discovering the thoracic duct, rendered the lacteal system, which had hitherto been questionable and deficient, indisputable and complete. He had the honor of finishing that new tissue of research which had been so prosperously begun.

Pecquet was led to trace the course of the lacteals with a greater degree of accuracy and attention than had hitherto been bestowed upon them, by observing accidentally a white fluid, resembling chyle, in the right

side of the heart of a dog, which he had been examining, perhaps with no view more remote than that of ascertaining the result to which it led.

The observations of Pecquet, Asellius, &c. were improved by the additional remarks of Jolyffe and Thomas Bartholin; but it was the work of after-ages to perfect and enlarge that system, which was as yet known only in its infant state. The discoveries of the ancient anatomists were limited only to observances on one department of the subject. It remained for more modern times to furnish that state of our science, in this particular, which is now grown into a plentiful harvest; a harvest which every year yields forth the fruits of accumulating abundance.

In the list of the more recent investigators of the anatomy and physiology of the absorbent system, the names of the admirable Haller, the laborious experimentalist Hewson, Cruikshank, Hunter, and a few others, stand *pre-eminent and illustrious*. And there are not wanting those who have signalized themselves in the present day by the ingenuity and zeal with which they have laboured in the cause.

If any apology were required for the tardiness which marked the progress of ancient discovery, we should have occasion to look no further than to the defective contrivance of the instruments which (for want of better) it was necessary to employ. The consideration of the converse would seem to diminish something from the credit which is due to the rapidity with which modern

results are determined. We must not forget, however, that our best thanks are due to the discrimination of those who conceived the means before they entered upon the business of inquiry; to those, who made the perfection of their instruments precede the attempts which they were designed to facilitate.

Having thus sketched the names of those to whom we are most indebted for the information they have transmitted to posterity, (a tribute of respect which could not have been justly withheld) we shall next take a view of the state of the science, as we find it at present. This view will include, first, the anatomy, and then the physiology, of the absorbent system. The pathology of every organ, or set of parts, is so immediately dependent upon structure and function, that the one can scarcely be entered upon at once, with fair advantages, unless it be prefaced by some introductory remarks on the others. And as structure is altogether subservient to the purposes of function, so the former claims the same precedence, with regard to the latter, as obtains between the relations of physiology and disease.

A Cursory View of the Anatomy of the Absorbents.

THOSE small tubes, whose office it is to convey a fluid called chyle from the intestines of an animal into the sangineous circulation, have been hitherto designated by the appellation of lacteal vessels. They were called lacteals, from the color and appearance of the fluid they contained, which was supposed to resemble milk. These vessels were the first of that system which is now collectively called the lymphatic or absorbent system.

It is most probable, that the mode in which the function is performed of the different orders of the absorbents is the same, and that a similarity also of structure obtains throughout. It does not appear necessary, therefore, that they should be expressed by any terms of contradistinction, unless it be defined that no other variety is implied by such terms than that which results from their different uses and applications.

The structure of a lymphatic vessel is somewhat analogous to that of a vein; it at least participates in that valvular peculiarity by which the veins are so prominently characterized. A lymphatic is composed of three coats; the first, an internal polished and inelastic coat, forming on each side of the caliber of the tube, at no precise distances, semilunar processes, or duplicatures,

which are said to serve the purposes of valves; the second is a middle or muscular coat, by the contraction of which the fluids contained in the vessel are propelled to their destination. The fibres of this coat, according to Sheldon, have principally a circular arrangement; but although this arrangement obtains the most generally, there are nevertheless fibres, which take their courses in all directions. The third or external coat is of a looser structure, and serves to connect the vessel with the contiguous substances.

It is difficult to separate these three coats of a lymphatic so as to render their distinction demonstrable. But although a single attempt at detecting them all at the same time may fail, we shall eventually succeed in ascertaining that such is its structure, by a frequent reiteration of the trial.

The muscular coat appears more evident in the absorbents of the lower extremities than in those of any other part; and a view of the others cannot choose but be obtained, where this is clearly made out. The internal coat of an absorbent vessel resembles the cuticular coat of an artery in the resistance it offers to the force of distention. The valvular processes of this coat are capable of sustaining a column of quicksilver, which no tube in the body of equal diameter can by any means support.

Hewson has informed us, that mercury will pass from an absorbent trunk of a fish into its branches; whence he infers, that the absorbents of fishes either want valves altogether, or else, that they are in them

inadequate to the performance of their usual functions. It is remarked, however, that this peculiarity is limited to such fish as are not furnished with lungs, and consequently do not respire atmospheric air. The valves of the absorbents in the turtle are easily demonstrable; they appear to be extended also to every description of fishes of the *whale class*.

The general distribution of the valves of the absorbents is in pairs, similar in this respect to that of the veins. But Nature, always provident of her means, deals but little in superfluities; she manifests the resources of her contrivance only where complicated provisions are required, by the difficulty or the importance of the end. Accordingly we find a deviation from the general plan of the valvular arrangement of an absorbent, at the point where a small vessel opens into a larger one; at such places the tube is furnished with only one valve, the lesser vessel being inserted into the larger in such a way, as to secure the design, which is otherwise accomplished by two.

The lymphatics possess very little elasticity; when air is impelled into them, without the employment of much force, they become distended to the extent of their capacity, and remain nearly in the same state of distention after the air has escaped. They are never found, in the dead subject, in a state of rigid contraction; the sides appear rather collapsed, and loosely approximated. The unyielding firmness of the internal coat of a lymphatic is indispensable, by reason of the offices of the valves. If it were formed of more complying materials, dilatations of the caliber of the tubes would be con-

tinually forming, at the places of the valves, thereby rendering the provision altogether ineffective.

Glands are every where interposed between the extreme branches and the trunks of the absorbents: their sizes are extremely various; some are of considerable magnitude, whilst others are so insignificant as to escape detection, except under a state of disease. Certain it is, that anatomists are indebted to morbid conditions for the developement of structures, both in this and many other instances, which would otherwise have eluded their observation. These glands, in some places, are found in clusters; in others, they are segregated, and more scantily dispersed.

In young animals, they are much redder than in those of maturer years, and their color is not alike in all the parts of the same subject. The texture of those absorbent glands, which are nearest the surface, as the inguinal axillary, &c. is capable of sustaining a higher column of quicksilver than that of the deeper seated glands.

We are informed by Rysh, Morgagni, and some others, that the glands of the mesentery *entirely* disappear in old age. Although we cannot choose but repose great confidence upon the respectability of such authority, individual observation must nevertheless invalidate the assertion as an universal law.

It would most probably prove a fruitless research, to inquire how far that state of the body which marks the period of advanced years is connected with, or

attributable to, the destruction of the glandular system of the mesentery. Experience and observation may throw some light upon the question ; but it is a point, the determination of which hardly falls within the sphere of practicable experiment. The instances of the disappearance of these glands are irregular ; and where the opportunities are abundant, it would be a series of observations not altogether devoid of interest, or perhaps the higher incentive, utility, to mark the different states of body, which are associated with such glandular varieties.

We are by no means acquainted with the functions of the mesenteric glands, and consequently we cannot determine how far they may be subservient to the purposes of nutrition. The leanness of old age, and that natural decay incidental to it, may perhaps be referable to this cause. But as our attention has never been specifically directed to the point, we do not as yet possess sufficient information to speak definitely upon it. It is entirely the investigation of experience and dissection ; and he whose opportunities for each are the most abundant, is the most likely to succeed.

An useless enumeration is made of the orders of the lacteals and glands of the mesentery. It would serve as good a purpose to distinguish methodically the fibres of a muscle. As we are hitherto ignorant of any function at all belonging to the absorbent glands, it is imposible to assign a physiological reason for their being so much larger in the infant than in the adult state. At the same time, the fact of their being so quadrates very well with the hint suggested above. We have reason to suppose

that there are no instances of mechanism and contrivance, more especially in the animal constitution, which are not subservient to an end; and the discovery of structure, where the end is not understood, should serve only as an additional motive for *physiological* research.

Some anatomists have believed the lymphatic glands to consist of mere convulsions of the tube of the absorbent. Others have conceived them to be of a cellular structure, with communications between the septa, by which they are intersected. The subject is too minute to admit of much elucidation from the labors of the knife. I have made some unsuccessful attempts at *unravelling* the structure of an absorbent gland. From these I am led to infer, that if the gland does consist of a convoluted tube, the folds are connected by a bond of union, of as strong a texture as that of the absorbent itself. Indeed, to determine this matter accurately, is beyond the means of the anatomist; and, for want of proof, we are obliged either to confess our total ignorance upon the point, or to indulge in conjectures, which every man will accomodate to his fancy or convenience.

The glands of the absorbents (save that peculiarity of structure which we cannot develope) partake of the general constitution of other organized parts. They appear to be a body of arteries, veins, nerves, absorbents, muscular fibres, and connecting cellular membrane.

Magnus Falconar, in the third part of his experimental inquires, directs our attention to some particles contained in the lacteal glands, which Mr. Hewson first supposed to be the central particles of the blood. This

was the discovery of the microscope; but the function which would appear to be implied by the observation is too conjectural, and too destitute of evidence of any kind, to merit much regard.

Lieberkühn was the anatomist who first saw the orifices of the lacteals. He has entered into an elaborate detail, more curious than useful, of the ampullæ, from whence they begin.* His account is extremely minute and correct; it was disputed by Hewson, and confirmed by Mr. Cruikshank. The opinions also of Doctors Hunter, Jebb, and Mr. Sheldon, go to strengthen the observations of Lieberkühn.

The general distribution of the absorbents follows the usual order of vascular arrangement. They consist of trunks and branches of various magnitudes, all tending to a centre. The absorbed fluids are not, however, conveyed directly to the heart, but arrive at it through the channel of some principal blood-vessels, which have proceeded but a short distance from it.

The largest of the absorbent trunks is named the thoracic duct. It is through this channel that the fluids absorbed from the intestines arrive at the left subclavian

* Ramusculus vasis lactei extenditur in ampullulam vel vesiculam ovulo haud absimilem, in cujus apice foraminulum quoddam exiguum microscopio detegitur. And, speaking of their vascularity, he says, Impleo in arteriam mesentericam materiem ceraceam tenaciorem, ita ut redeat per venam mesentericam sat magna quantitate. Examino dein microscopio quam plurimos villos: invenio in omnibus non modo distenta vascula, sed etiam turgere ampullulam lactei cera alba plenam, &c. *Dissert. Joannis Lieberkühn de Fabrica & Actione.*

vein, where they are mingled with the sanguineous circulation. The thoracic duct occasionally bifurcates at a short distance before its termination; it then opens into the subclavian vein by two orifices.

It is to be regretted that the more recent labors of Mascagni have had no better effect than to increase the store of our anatomical, without adding any thing to our physiological or pathological knowledge. Not possessing his magnificent plates descriptive of the anatomy of the absorbents, (which I have only once had an opportunity of seeing) I cannot, if I would, profit by his delineations. If, however, my recollection informs me truly, I feel assured that they would contribute but very little towards rendering this attempt more complete.

But little else remains to be said of the structure of the absorbents; and to express even that little, does not fall within the compass of my design. The physiology of this system will be next considered; and if any thing advanced in the course of the detail assume an air of novelty, to which it has no pretensions, I can only say, by way of apology, that I must be censured, not for a want of honesty or candor, but for the scantiness of that leisure which has so parsimoniously limited the opportunities of information.

On some Particulars of the Physiology of the Absorbent System.

THE food, when taken into the stomach, undergoes that process which is termed digestion. It passes from thence into the intestines, where it is again changed, by being blended with some visceral secretions. The food in this state is called chyle, and is now fitted for its reception into the body for the purposes of assimilation and nutrition. So much of it as is capable of contributing to the support of the animal is taken up from the intestines by the lacteal vessels and conveyed by them into the sanguiferous system. The matter which is absorbed from the intestines, immediately on leaving the thoracic duct, is conveyed into the right side of the heart, and from thence to the lungs, for the purpose of oxygenation, or some change which is commonly designated by this term. Having undergone this change, it is then constituted blood.

According to the (even now) very generally received notion of the functions of the lungs, the blood is subjected to no other agency in these organs than that which is exerted upon it simply by its exposure to atmospheric air.

It is a fact, which the authority and opinions of the best physiologists must render indisputable, that the component parts of the body are undergoing a constant series of changes. No person will doubt the fact; although there might obtain a great diversity of opinion concerning the precise period which is required for a total substitution of new, for the original, matter of the body. The new matter of the body is supplied from the intestines; and the chyle in the absorbents of the mesentery is white: whence then does it derive its change of color? It is said to be produced by its mixture with the blood. But the blood is *totally* renewed within a definite time; and if the alteration in the color of the chyle is assignable to this cause, in proportion as the blood is wasted and renewed, it ought to become progressively pale until it had attained to the whiteness of the chyle itself. We must look then for some *source* of colorification—and where will it be found? Certainly not in the lungs, according to the usual explanation of their function.

The combination of oxygen with the blood may change its appearance from a dark to a florid red; but I conceive it necessary that the lungs should possess an ability of accomplishing the same end by a process altogether independent of atmospherical influence. If the chyle be exposed to the operation of the atmosphere in the lacteals of a dog, the color of it remains unaltered. It is evident, therefore, that the capability of perfecting chyle into the state of blood resides in some *latent* function of the lungs, or some other organ; or else, that it is brought about by a process which has not as yet been adverted to.

I am not acquainted with the full extent of Sir Humphry Davy's experiments. He supposes, however, as far as I can collect, that the principle of vitality, conferred and transmitted by the nerves, is of an identity with electric matter. It would be worth while to subject the chyle to the electric agency, to observe the effect of that influence upon it which is supposed to be analogous to the principle of life. It is only *supposed* to be analogous, it certainly does not as yet merit a higher regard; and, even as a supposition, it appears extremely crude, liable to insuperable objections, and by no means warranted by any of the data which have been premised. If such were the fact, it ought to be evinced by a manifestation of the various affinities, properties, and phenomena at other times exhibited by the matter of electricity. Hence I should entertain no very sanguine expectations of the success of the experiment hinted at.

I have supposed it necessary that the lungs or some other organs should possess a function independent of the atmosphere, by which the color (and perhaps some other properties) of the chyle is changed and assimilated to the nature of blood. It is mere matter of conjecture that this function resides in the lungs: we are warranted to infer that it exists somewhere; but it is the business of future experiment to determine to what organ it belongs. It is an interesting, and I think will one day prove a highly productive theme of inquiry, to investigate the physiology of the colorification of the animal fluids, beginning from their first formation in the mesenteric absorbents, and tracing from thence the varieties of color incidental to them in the different orders of circulation.

In the course of this inquiry we should notice, first, the change of chyle into arterial blood, and we should be highly interested in the explanation of the manner and means by which this change was accomplished. We should next remark that some of the arterial blood flowed to the remotest parts of the body, and still retained its florid red, and all the other properties distinguishing it from venous blood. On the other hand, we should again observe it becoming black, and assuming the properties and appearance of venous blood, when it had proceeded but a very limited distance from the heart.

The blood, for example, in the arteries of the extremities, is of the same color as that in the aorta; and the blood conveyed to the spleen through the splenic artery has no sooner reached the extremity of a vessel, which has proceeded but a few inches from its trunk, than its color is changed, and it becomes venous blood.

We should infer, from these facts, either that the different vessels themselves (arteries, veins, &c.) possessed a variety of function or peculiar power of operation upon the blood, from which these effects may result; or that the veins receive blood only after it has been subjected to the processes of the secerning extremities of the arteries; and that the color is changed, not by an uniform extrication of oxygen for the purposes of animation, as some have supposed, but by an agency of secretion which must be common to the secerning organs with which every point of the body is furnished. This supposition is rendered extremely probable by collateral evidence and considerations.

The arteries terminate part in secerning extremities, and in part by opening into veins. There is made by some a more elaborate detail of their terminations. It is concluded from the results of mercurial injections, and from microscopic observations, that the terminations of arteries are in red veins, in glands, in cells, into which red blood is effused, in lymphatic veins, and in exhalents. But this formal distinction does not invalidate my assumption. Red blood is conveyed to the secerning extremities; part of that red blood is removed from it by them, and there must remain a further continuation of vessels capable of transmitting the blood which has been subjected to the process of secretion. That blood then is either wholly or in part propelled through the veins which has had some of its constituents abstracted by secretion, and upon those abstracted constituents the properties might depend which distinguish arterial from venous blood.

If I am correct in this assumption, it follows that the re-combination of the abstracted constituents ought to convert venous into arterial blood. The matters of absorption are conveyed into the sanguineous circulation at a point immediately contiguous with the heart. Is it not possible that such matter may not be completely mingled with the blood until it has performed the pulmonary circulation? until it has been subjected to that compression alternately remitted and employed, which must necessarily take place in the lungs, and which seems admirably calculated for the end, it is conjectured, might be thus accomplished?

The oxygenating theory is now, to say the least of it, rendered extremely dubious; and it is but fair to seek for an explanation of the phenomena wherever there appears the remotest chance of finding it. The fate of this surmise may be determined by an experiment; but I have at present neither time nor opportunity to enforce it. Pecquet was led to the discovery of the thoracic duct, by observing in the right side of the heart of a dog a white fluid resembling chyle: a sufficient proof that the chyle, although carried into the sanguineous circulation at the point of junction between the subclavian and internal jugular veins, is not completely mingled with the blood even in the heart itself. From these considerations it appears necessary to conclude, either that the lungs have the power of converting chyle into red blood without any atmospherical influence, or that they do not possess the property of producing any change in it at all. If the latter alternative from any future evidence should be adopted, it will be necessary to seek elsewhere for a solution of the difficulty; and I think it may be explained by referring to the changes which the chyle undergoes in the course of the circulation. It is easy to conceive that the properties of a fluid may be changed as readily by an abstraction of its constituents, as by combination with new matters. We have seen that it is necessary there should be some *source* for the colorification of chyle. This fluid passes into the sanguiferous system: it performs a circulation by which it is subjected to numerous secretory processes. These processes deprive it of the properties of chyle, and convert it into

blood. One part of the secretion is appropriated for present animal purposes, while the other is transmitted from the absorbent trunks again into the sanguiferous system, where it is mingled with that venous blood which has undergone secretion, *partially* restoring it to the primitive color and properties of chyle. This is meant as no better than a mere speculation; and it must be allowed that there is no subject wherein speculation may be more appropriately indulged. Thus much also may be said in recommendation of it, that it appears very consonant with the consequences of the abstraction of blood, and all the leading phenomena belonging to this fluid.

To pursue this topic any further would be here altogether irrelevant and misplaced; it may be observed, however, that if these assumptions were established, on a more minute investigation, and rested upon a better evidence than mere conjecture, the inference of the necessity of a secreting process, in order to convert arterious into venous blood, may possibly reflect some additional light upon the function of the viscus quoted in the illustration. At a future period and in another place I shall probably resume these considerations. The field is an ample one: and a great deal remains to be discovered in a department which is at present only partially and faintly illumined, by the occasional rays which the labors of individuals have shed upon it.

On the Agents of Absorption.

THERE are few surgeons who have not, at some period of their professional career, been much perplexed to understand that part of the function of the absorbents which may be denominated their modus operandi; and every man must have conjectured for himself upon this point, who feels pleasure in scrutinizing those minute processes by which Nature elaborates some of her most beautiful works; and who likewise delights in an acquaintance with all that belongs to those agents which he is so frequently called upon to employ. The subject is involved in the darkest obscurity; and, unfortunately, both in this and numerous other instances of ultimate operations, hypothesis must take the place of better proofs, and theory must suffice where the light of experiment is precluded by the minuteness of research.

Absorption is a process in an animal body, which is unremittingly employed. There is a diffused and perpetual secretion going on, and this process must be every where balanced by an equal activity of the absorbent system.

The question naturally suggests itself, of what use, and what end is fulfilled in the animal economy by the perpetual absorption of matters, which are first separated from the blood only to be again conveyed into it? In default of a more essential purpose, (the discovery of which appears not altogether hopeless) it is sufficient to say, with Mr. Charles Bell, that the animal body is a machine every where designed for motion, and that the mobility of the parts is facilitated by the effusion of an interstitial fluid. Notwithstanding every point of organization is permeated by fluids contained in tubes of proportionate caliber, some provision of this kind is required; since, without an exhalation or something tantamount to it, parts would be undergoing a harsh and continued friction; and if the effusion of a fluid be necessary for a purpose in the animal economy, the absorption of that fluid in a ratio to its secretion must be equally so.

We find that the action of the absorbents is not confined to the taking up of fluids: the solids are undergoing a continual change by the intervention of the same agents: neither is their operation limited to any densities of structure; but all, from adipose to osseous, healthy and morbid, substances, alike fall within the sphere of their agency.

It is necessary to conclude that the absorbent tubes exist every where in the body, and that there is no point of it which is not plentifully furnished with them. We must suppose also that the absorbent trunks terminate, like those of the veins, in minute ramifications ending in

open mouths; we otherwise could not explain the absorption of extravasated fluids without recurring to an ulceration of their coats; and even this resource would not in all instances serve us.

We have no reason to believe that any direct communication, such as would be produced by a continuity of tube, subsists between the absorbents and the veins. But there is more than reason to suspect that these different orders of vessels have a similarity, to a certain extent, both of function as well as of structure.

The ingenious experiments of Mr. Brodie* upon this subject would lead to the conclusion, that absorption takes place exclusively from venous orifices, and that the absorbents themselves have no participation in the process of conveying the matter of inoculation from the surface into the general circulation of the blood. This result of his experiments appears of no small importance as a point of physiological information. It may not be altogether misplaced to bestow a comment upon the conduct of the experiments, and the legitimacy of the conclusions inferred from them.

Mr. Brodie injected alcohol with tincture of rhubarb into the stomach of a rabbit: the animal died in about half an hour. Mr. B. concludes that the *absorption* of the spirit from the intestines had not taken place, because

* Dr. Magendie, of Paris, has, by recent experiments, been led to a somewhat analagous inference.

the rhubarb was not discoverable in the urine of the animal. He appears warranted by this experiment, in the inference, that the spirit destroyed the functions of life by immediate propagation from the nerves. The experiment, however, is not entirely conclusive, as it is not impossible but the spirit may have been absorbed before the rhubarb: the objection acquires some little sanction from considerations elsewhere noted. Mr. B. inoculated a cat with the woorara, and the subject of the experiment died shortly after, although the lymphatic trunks were tied, through which it was necessary the poison should pass, if it were absorbed by these agents.

He also divided the axillary plexus, and inoculated the leg of an animal: the limb was paralysed and senseless, but the subject of this experiment died in ten minutes. The inference which would appear to be warranted by this experiment cannot obtain an unqualified assent. There is a connection subsisting between every part of the body; and though the functions of a limb may be lost by the division of the principal nerves, a nervous communication must nevertheless subsist between the place of the wound and the brain. The reticulated expansion of nervous filaments every where existing, and appearing to form a connection every where continuous, might be capable of transmitting any thing which it is the peculiar property of the nerves to transmit; but might nevertheless be inadequate for the purposes of sense and voluntary motion. Mr. Brodie has no right to conclude from *this* experiment that the nerves

did not in any degree participate in the production of the result; such a conclusion is, however, warranted by a subsequent part of the detail.

The veins of a limb were tied; the woorara was smeared upon a wounded surface; but it remained perfectly inert. This experiment was frequently repeated, and always with the same event.

It would appear, then, that absorption sometimes takes place from the veins, and sometimes that death follows the inoculation of poisons, from the exclusive properties and intervention of the nerves. We are furnished, by this inquiry, with no reason for supposing that absorption from the surface is performed by the agency, or even participation, of the absorbent vessels.

Mead relates a case in which death ensued from the inoculation of the poison of the viper, although it is specifically mentioned by the experimentalist, Dr. Areskine, that the wound had ceased to bleed before the poison was applied. Is it not fair to suppose that blood would continue to flow upon a recent surface until the venous orifices were rendered impervious? There is much reason for believing that such is the fact: similar in this respect to the spontaneous cessation of hemorrhage from the ramifications of an artery. If this supposition be conceded, it will be impossible to conceive the manner in which the absorption took place in this instance, unless we recur to the doctrines on this head which have hitherto obtained.

We are not informed by Mr. Brodie, if my recollection serves me, what length of time the animals were suffered to live subsequent to the experiments of rendering their veins impervious by ligature. This is a particular of no small importance. If absorption be entrusted wholly to the function of the lymphatics, we have reason to believe that the process would be extremely tardy. The free and pervious state of the veins may possibly facilitate absorption, although their orifices should be obliterated, if we suppose that the absorbents of the veins have any communication with the caliber of their tubes.

It is most probable that the absorbents are exclusively capable of taking up fluids deposited upon an exposed surface. It is also more than possible that the process is facilitated where there has been a division of small venous branches by a communication subsisting between the absorbents of these vessels and their cavities. The absorbed matter would in that case be immediately mingled with the blood, and would obtain a more rapid circulation to the organs essential to life, than if the business of its absorption were rested entirely upon the lymphatics.

The cause of the death of the animal in that experiment wherein the lymphatic trunks were tied, is probably this—that either the venous orifices were pervious, and transmitted an oozing o f blood, or else that the woorara was propagated from the absorbents of the

veins immediately into their cavities. The conjecture of this supposed communication is not altogether unsupported by real occurrence. How else are we to account for the absorption of fluids which, perhaps, invariably takes place in the instance of a vessel obliterated by ligature or compression?

When the veins were tied, it is said that none of the effects of the poison were manifested; but, as I before observed, we are not informed what length of time the animal was allowed to survive in order to ascertain this point. If the communication I have assumed be admitted, it would be easy to conceive how the absorption was retarded; and it would militate strongly against Mr. Brodie's conclusion that the lymphatics themselves take no part in the process. It appears highly probable that some future observations and experiments will bring our knowledge, in this particular, back to the point from whence it has, by Mr. B.'s inquiry, so heterodoxically digressed.

On the Modus Operandi of the Absorbents.

THE process or manner of absorption has been a matter of much conjecture; and it is astonishing that so *few* theories have been broached upon a point which must have obtained so general a regard.

The one perhaps which, for want of a better, is the most generally embraced is this, that fluids, &c. are taken into the mouth of an absorbent by the force of capillary attraction. We know so little about the anomalies and rationale of this power, that the more seldom it is introduced into our explanations the better. It seems an universal dernier resort, a never-failing forlorn hope; and it serves only to preclude a more extended view by filling up the gaps and crevices of research. Some ultimatum of this kind becomes necessary where our attempts to proceed further are baffled by the obstacles and intricacy of the road; but let us never resort to it until all other means of surmounting the difficulty have failed.

The absorbents are furnished with a muscular coat, by the aid of which, it is supposed, the fluids they contain are transmitted from the branches to their trunks. If this muscular coat of an absorbent vessel take any part in the propulsion of its contents, it is necessary to infer a pulsatory action. An uniform contraction of this coat would rather resist than facilitate circulation; the muscular fibres therefore of an absorbent can lead to this effect only by an alternate contraction and dilatation.

It appears possible that the business of absorption may be accomplished without recurring to any other property or peculiarity belonging to them, save that pulsation which we have just insisted upon. The absorbents, as has been said, terminate, or rather commence, every where in open mouths. Fluids lying in contact with, or pressing upon these absorbent orifices, would stimulate them to contract; and the pressure which must be exerted upon any extraneous matter would apply a new surface to the absorbent orifice.

But we observe, it will be urged, that bone is frequently removed by the absorbents, and that it occurs when we have no reason to suspect a previous decomposition or breaking down of its structure. This, however, is hard to be conceived, and in no instance can it be supported by an evidence which demands assent.

The contraction of a muscular fibre is produced by the action of a stimulus upon its irritability, (or, if

accomplished in any other way, it is equally good for my purpose,) and the state of excitement thus produced would be sufficient to induce the same contraction, although it resulted from the operation of other means. The permanency of the contraction must depend upon the continuation of an adequate supply both of stimulus and irritability.

If an absorbent vessel were in a state of increased excitement from causes arising out of its own constitution, and without the aid of extraneous stimuli, the effect of that excitement would be to increase the energy and rapidity of the actions peculiar to it.

It appears very probable that vascular contraction and dilatation result from an irregular supply of nervous power, a supply alternately communicated and withheld. If we suppose the converse of this, in an inflammatory diathesis prevailing throughout the arterial system, the arteries would rigidly contract upon their contents; and the presence of the blood, if that be a stimulus, would be only an additional incitement to contraction. The heart itself would be peculiarly liable to this condition. The blood flows into it from the veins only by the impetus which it acquires from a vis a tergo; and if the power necessary for a permanent contraction were communicated and maintained by a process of inflammation, that contraction would be sufficient to impede effectually the entrance of the blood; seeing that the blood flows in an uniform current through the veins,

and that it requires no greater force of contraction to obstruct and resist its entrance into the heart at a subsequent, than at a preceding moment.

Granting then that a vessel may contract, if it be stimulated to do so by actions going on within itself, which would raise its excitement to as great a degree as that which ensues from the operation of a foreign stimulus; conceding thus much, we must suppose it possible that, under some morbid affections, a continued reiteration of contraction and dilatations might ensue from an accelerated, though still an irregular, supply of nervous power: at other times, the same may arise from the stimulus of extraneous matter.

The effect of this pulsation upon an absorbent vessel would be to produce a vacuum at its orifice; and this vacuum, from the surrounding pressure, would have all the effect of a forcible attraction.

I cannot, however, conceive that the absorbents can possess the power of removing solids, unless the solid particles are decomposed and fitted for the function by a previous process of disease. I cannot conceive this possible, whatever that modus operandi might be which it is determined to assign them. We know so much of attraction as to be assured that no practicable degree of it is adequate to destroy the cohesion subsisting between the particles of an animal solid. We have reason to believe that an exhausting syringe (which is in effect a

powerful attraction) might remain upon a surface to all eternity, and that the cohesion of the parts of that surface would never be destroyed by such an agency, except by the supervention of disease.

In many of the instances of absorption of the solids, there is great reason to suspect a previous modification of vascular actions, inducing the decomposition of structure, and in no individual instance can it be proved that such previous disease has not obtained.

The solution which I have hazarded merely as a query, appears objectionable on many accounts; and the result of tying an artery, and the cessation of the pulse between the ligature and the inferior anastamoses, would appear to form its complete refutation. The effect of the ligature, however, may be not only to prevent the circulation of blood through the tube, but to cut off likewise the transmission of nervous power, which is again restored where the circulation of blood through the vessel is resumed. I do not feel disposed to lay much stress upon this explanation, and the more particularly as the one more commonly adopted will serve my purpose as well.

The stimulus of distention is usually assigned as the cause of arterial contraction: it appears that the greater the irritability of the vessel, the smaller is the quantity of the fluid necessary to produce its contraction. This state of the arteries must obtain in fever. I have

supposed, upon grounds before stated, that disease precedes the absorption of a solid. The particles composing it occupying, when separated, a greater space than when densely compacted, as in the state of health, must become a source of irritation on the absorbent orifices: hence a contraction; and, in the way that has been explained before, a repetition of contractions and dilatations. This process is of course modified by the varieties of irritability incidental to the absorbents of different subjects, or of the same individual at different periods.

If the action of an absorbent depend upon any thing like capillary attraction, this attraction should be uniformly operative, and we should be furnished with no clue for explaining the irregularities of activity incidental to the function. It is true, we may suppose that some morbid conditions of the absorbents may alter their function even in this way; but we cannot conceive that it could happen except from a change of structure. The sudden transitions of their actions, and all the leading phenomena of absorption, rise up in evidence against this subterfuge.

If we can explain the action of the absorbents at all, it is only by a reference to their constitution, as partaking of the same irritability, and the same principle of animation, as are assignable to other organized parts, modified only as much as is subservient to the end of their proper function.

The subject of absorption has afforded one of the most striking instances of the extravagance and absurdity to which an inordinate rage for explanation might lead. It will be immediately anticipated that I allude to the doctrines and hypotheses on this head which were so romantically entertained by Mr. Hunter. To rest satisfied with total ignorance was altogether incompatible with his active and inquiring genius; and when we consider this propensity and the enterprising boldness with which he was always ready to attack the most recondite and impenetrable topics of research, we cannot feel surprised that he should have indulged in the wild sallies of imagination, to illumine that gloom upon which the efforts of reason and the patience of experimental investigation could throw no light.

Mr. Hunter concluded the absorbents to be animals, capable not merely of eating and drinking; endowed, not simply with a vegetable vitality; but animals of an higher order, possessing intellect! and that intellect furnished with a surprising fund of information, and a sagacity which it would be in vain for the grosser faculties of human understandings to emulate!

It would be necessary that this new species of animals, these same absorbents, should have been furnished with senses of inconceivable refinement, in order that those myriads of notices may be conveyed to their minds of occasions wherein their offices were required for the good of the body; and which, it is assumed, they would

then laudably execute upon nothing short of a rational and moral conviction!

It has been said that a balance obtains to a certain extent between the processes of absorption and deposition; that a diminished activity of the one function admits an increased effect from the other, and *vice versa*.

To allow this principle would certainly afford room for a tolerable explanation of many phenomena which appear difficult to be accounted for upon any other. It is not, however, altogether free of objection. An accumulation of animal matter would thus result from a preponderance of the vigor and action of the arterial over that of the lymphatic system; and, in a general affection, leanness would ensue from an increased excitement of the absorbents, while that of the heart and arteries was reduced. In order to preserve the uniformity of condition in this particular, it is unquestionably necessary that a counterpoise should subsist between absorption and regeneration. But this view of the matter nevertheless involves some little contradiction, as is sufficiently evinced by the phenomena of fever; unless it be supposed that the actions of the two systems have no mutual participation, but run their courses under the influence of exciting causes altogether independent of each other.

It is necessary to conclude that the absorbent and producing powers are furnished with different irritabilities, and that either is liable to be primarily and ex-

clusively affected, in some instances by the same, and in others by different stimuli; we can then conceive how one inflammation may thicken, and another waste the animal structure. There subsists between these powers such an interchange of excitement from stimuli of about the same degree, that we cannot assign to either a greater share of irritability than it may be supposed is possessed by the other; except either be independently affected by disease. These are points, however, which will form a subsequent consideration; and will be more properly introduced when our regards are more strictly bestowed upon the pathology of the absorbent system.

One would hardly conceive it possible *a priori*, that the process of absorption admits of those irregular periods for its occurrence, which we should otherwise be led to infer from the observation of some facts. A chancre will remain on the prepuce for a considerable length of time before a constitutional syphilis becomes manifest. The same thing sometimes happens in the instance of a wound inflicted by a rabid animal.*

On the other hand, we see that death follows the bite of a serpent in a few hours or less after the application

* It has been supposed of late that no matter of any kind is *absorbed* in this case, but that the disease termed hodrophobia is produced merely by the terror and apprehension of its occurrence. I would ask how, in the primitive instances, could the dread of the supervention of hydrophobia *arise*, before the disease was known?

of the poison. The process of absorption, whatever may be the agents employed in it, should be conducted in periods of an occurrence as regular as the actions which are subservient to the end. This must unquestionably obtain if absorption be the exclusive office of the veins.

When matter, capable of producing a specific constitutional disease, has once gained admission into a vein, and is mingled with the blood it contains, that matter must of necessity obtain an immediate transmission to the heart. If it were allowed to rest in the cavity of the vessel, the blood which would otherwise urge its circulation must be in a state of rest. Coagulation of this fluid would next result: the vessel would then be plugged up and obliterated by the coagulum, and altogether incapacitated for the absorbent function. On this ground we may, I think, affirm it to be impossible that absorption from the surface is, as Mr. Brodie is led to conclude, the exclusive business of the veins.

At the same time, although this and some other observations would appear to militate very strongly against the result of the experiments already cited, it must nevertheless be allowed that the veins participate very decidedly in the process of absorption; so much so, that it appears impossible that this end can be speedily accomplished without their intervention, except in those instances wherein it may be supposed the function is assumed by the nerves.

In taking a retrospective view of the evidence before us (which appears sometimes a little parodoxical) I should incline to the following explanation of the phenomena in question.

It appears probable that the veins possess the power of absorption; that they can exercise this function only when their tubes are partially divided, and when the blood may flow through them without being effused from the wound. When matter is absorbed from the veins, the period of the process must be uniform; it must find immediate admission, or the orifice would in a short time be closed by coagulum; and when once within the cavity of the vessel, it must be propelled with a quick, but regular progression to the heart. This progression in venous absorption cannot be accelerated, because, however stimulating and acrimonious the properties of the absorbed matter may be, the veins cannot be made to pulsate; and there remains no other mode of obtaining an accelerated transmission. The effect of tying the venous trunks would be to preclude altogether this mode of absorption; which, it is but fair to conclude, is one of greater rapidity than some others.

If the point of a lancet, impregnated with matter capable of producing a specific disease, be introduced between the cuticle and the skin, (and this might be done without wounding a blood-vessel of any kind) the matter will produce the constitutional affection peculiar to it, as certainly as if it were already diffused in the circulation.

Absorption then is also the function of the lymphatic system. The lymphatics are furnished with a muscular coat, and there is, as has been already noted, more than presumptive evidence for believing that they propel their contents by a pulsatory action. It is no very outrè conjecture to suppose that some poisons, whether morbid or otherwise, may possess a higher property of stimulation than others.

The effect of the action of a stimulus on a pulsating tube would be to produce a more frequent reiteration of contractions and dilatations, as the pudic artery and its branches are affected by the stimulus of desire. Hence anomalies in the periods of absorption might result from varieties in the stimulating properties of the absorbed matter. The absorption of the same poison may be marked by irregular instances of time in different individuals, owing to the prevalence of different idiosyncracies affecting the absorbent system. Another source of the irregularities of period between the introduction of the poison and the occurrence of its constitutional symptoms may be found in the different idiosyncracies of bodies not peculiarly incidental to the absorbent system, disposing some for a speedy, and others for a tardy derangement of functions.

An aura subtilis (or something which, for want of a better, may be expressed by this term) may also be propagated from surfaces to the brain, through the intervention of the nerves. We have reason to believe that this

does sometimes happen, and the experiments quoted, indeed, put it almost beyond a doubt; but we are furnished with no certain and universal test of discrimination by which to ascertain when their agency is or is not employed. It is probable that when the nerves are principally concerned in the process, the application of the matter, and the influence of it upon the body, are almost synchronous. It may be supposed also that the first evidence of disease would be manifested by a primitive derangement of the function of the brain. But this criterion is not worthy of entire confidence, because we cannot define a precise limitation to the agents which do, or do not produce their effects on the body, by a primary appeal to the same viscus.

The limits proper to be observed in this essay will not allow a more elaborate discussion of the *physiology* of the absorbent system. Many important particulars are not expressed, and numerous difficulties remain unexplained. By way, however, of reconciling myself to these omissions, I am inclined to believe that to pursue the argument any further would only be to involve the subject in still greater perplexities; and would ultimately have no better effect than to prove it to be in the minutiæ of its considerations altogether inexplicable.

On the Pathology of the Absorbent System.

THE absorbents, speaking of them inclusively as a general system, may be said to be subjected to diseases of action and diseases of structure. The former are constituted either by deficiency or excess of excitement, inducing the diseases dependent upon a diminished, and an increased, energy of their functions. The latter can become operative only by establishing one or the other of the conditions incident to the former. It is by a manifestation of the effects of an imperfect function of the absorbents, that we are enabled to infer their participation in disease.

The diseases of the absorbents must frequently arise from an idiosyncracy or pristine peculiarity of constitution, excited to the establishment of morbid conditions, sometimes without any assignable cause, but merely from a continuance of the actions by which the life of the animal is sustained. We are necessarily led to this conclusion by observing that neither the force of similar habits, nor the operation of similar agents of casual application and occurrence, are characterised by

the same results in different individuals. There are perhaps but few forms of the diseases to which the absorbents are liable, which admit an explanation without reference to this original predisposition.

This predisposition, in conjunction with other exciting causes, must be regarded as the remote, and the effect of this condition of the absorbent vessels upon the functions peculiar to them, as the proximate cause of many of the diseases which arise from the defective offices of the absorbent system. It is this modified *function* which will be principally regarded in the subsequent considerations.

The absorbents may become diseased either locally or constitutionally; and the state of disease in either may be either primarily or secondarily induced; but their affections are so complicated with the processes of secretion, that the nicest discrimination is sometimes insufficient to define the distinction between them.

Having premised these few general observations, I shall next proceed to the particular consideration of some of those diseases, in the production of which the absorbents appear materially to participate.

Scrophula and Emaciations considered with relation to the State of the Absorbent System.

THE diseases spontaneously affecting the absorbent system are generally symptomatic. The one perhaps of the most striking and frequent occurrence, is that producing emaciation of the body. This very frequently arises from a morbid state of the glands of the mesentery, induced by a scrophulous inflammation, to which they appear peculiarly liable.

The effect of this inflammation is to alter the structure of the glands in part, by an effusion of coagulable lymph, and in part by the establishment of an imperfect suppurative process. These processes lead to an organic enlargement of the bulk of the glands, and also to a state of them in which they contain that kind of matter, by which scrophulous suppuration is so unequivocally characterised. These actions do not always obtain throughout the whole glandular system of the mesentery; it most commonly happens, however, that the whole of

them participate in a greater or lesser degree, although the tendency has sometimes a contrasted effect upon the individual glands.

The greatest part of them are found enlarged, and in the state described above; while others among them are of a diminished size, and perhaps some altogether removed. The lymphatic vessels are themselves furnished with absorbents, as well as with arteries and the other components of organized parts. It is necessary to infer that the inflammation above noticed is operative upon the structure of a lacteal vessel or gland, and it is easy to conceive that the same action may produce different results, by affecting specifically any *one* order of the parts by which it is constituted.

If an inflammatory action were set up in the arterious structure of a lacteal gland, it would be followed by the effusion of coagulating lymph, or the secretion of matter, or both. If, on the other hand, this action were limited to the *absorbents* belonging to the gland, the function peculiar to them would derive new vigor, and they would ultimately remove all that matter which may fall within the sphere of their influence. This forms an useful distinction in the explanation of some of the apparently contradictory phenomena of inflammation.

There is a perpetual waste and consumption of both the fluids and the solids of the animal body: the deficiencies

produced by the constant and complicated series of actions going on in it are supplied by the absorption of chyle from the intestines by the lacteal vessels. These vessels are liable to become thickened and obstructed by a scrophulous inflammation. The glands appear the most disposed to this inflammation; and the glands are either formed by convolutions of the lacteal tubes, or else these tubes are merely continued through their substance. Which ever conclusion be adopted, it is alike easy to conceive how inflammation, leading to thickenings of structure and the accumulation of adventitious matter, may be followed by a total glandular obstruction. From a cursory regard of the matter, it would be difficult to conceive that life should be any longer sustained when a total obstruction of all the mesenteric glands had occurred; on the other hand, it would appear, if their office of transmitting nutritious matter were only in part impeded, a diminution only in the bulk of the body would ensue.

It has been supposed, by some, that this diminution is attributable to the diseased state of the mesenteric glands, in as much as it renders them incapable of continuing that function upon the matter of nutrition, which, it is conjectured, they at other times exert. This involves a supposition which can in no wise be substantiated by any admissible evidence; at the same time there is no necessity for recurring to it, as the circumstance may be explained in another way. The lateral anastamoses may be in part adequate to the transmission

of nutrient matter; but it is impossible to determine whether the body receives, by these agents, as great a supply under this state of disease as is conveyed to it in the condition of health. This last is the most natural conjecture, and it appears to be borne out very strongly, at least by the arguments of analogy.

Whatever may be the effect of diseased mesenteric glands upon the chyle, we are warranted in assigning this as the most frequent cause of bodily emaciations, seeing that the two states are almost invariably associated.

A condition somewhat analogous to that which we are considering, a sort of constitutional leanness, may result from an unusual vigor and energy of the general system of absorbents, independent of any affection of those which are denominated the lacteals. But this tendency, under some restrictions, cannot produce any great or rapid excess of waste, as absorption can then become only the counterpoise of secretion; and as long as it does not lead to organic disease, the health of the system may be sufficiently preserved.

It has been supposed, by some respectable medical authorities, that a partially impervious state of the mesenteric glands is essential for the production of that rapid waste which occurs in the disease of pulmonary consumption. My own observation enables me to refute this conjecture as an universal law. The circumstance admits an easy explanation by attributing to the absorb-

ents in general an activity which exceeds that from which only leanness might ensue, in as much as it is greater in them than in the organs of secretion. It is that state of them which I have supposed capable, under some circumstances, of removing the solid parts of an animal by the rapid and forcible reiteration of their actions.

The diseases of a scrophulous habit appear so particularly incident to the absorbents, that they are sometimes found filled with the matter of scrophulous suppuration.

The seat of this disease is not confined to the glands of the mesentery: it frequently attacks those of the axilla, groin, neck, &c. though in these parts it operates less to the prejudice of the health and constitution than when manifested in organs subservient to a function of higher importance in the animal economy.

The inflammation by which these glands are attacked is of the chronic kind, advancing to a slow and imperfect suppuration. This, however, is not the constant termination of the morbid action; more glands than one are commonly affected, and a whole congeries of them are frequently, by the thickening and agglutination of the surrounding substance, compacted into a tumor, presenting to the touch the feeling of an uniform surface. This state of the absorbent glands frequently gives rise to an erroneous conclusion concerning the exact locality of the disease; and in no instance is this mistake more

likely to occur than in the case of a supposed enlargement of the submaxillary gland.

All these instances and varieties are generally referable to one common cause, viz. the actions which result from what is called a scrophulous tendency.

When the mesenteric glands are principally interested by this tendency, we can do little more than regret the imperfections of that art which the exertions of genius, in regard to scrophulous disease, have been inadequate to improve. It depends upon a constitutional cause; and that cause no remedies, either empirical, or dictated by principles which the effects have proved to be chimerical, have hitherto enabled us to subvert. We are entirely ignorant of a curative treatment of this opprobrium of our art; at best, our attempts can have no higher felicity than that of mitigating, and partially averting, the evils which we cannot entirely control.

It is of the first importance in the case of diseased mesenteric glands to ensure the health of the chylopoietic viscera and the proper digestion of the food. By such treatment, we remove a *concurring* cause to the prejudicial effects of this state of the glands upon the constitution of the subject. We afford an opportunity for as much nutritious matter to be carried into the system as is compatible with the existing disease. While the functions of the alimentary canal are subject to considerable derangement from the same cause, it is but fair to conclude

that the morbid condition of the lacteal glands would be increased by the imperfect offices of the digestive organs, a re-action being thus set up and established between them.*

It would be an useless and perhaps an endless detail, to enumerate all the variety of remedies which have been at different periods proposed for the cure of scrophula. It must suffice to say that they have never been found to succeed, except in the hands of those by whom they have been recommended. It would also be digressing somewhat too widely from my purpose to bestow any considerations upon a disease which does not form the subject of my present regards.

When, however, those glands are attacked by it, which merit the attention of the surgeon, two indications must arise, from the varieties by which its progress is characterised. The first is to diminish the local actions; which, though of a specific kind, are nevertheless inflammatory: the second is to produce the absorption of adventitious matter, when the increase of excitement has subsided.

* In an impervious state of the lacteal system, would it not be possible to convey sufficient nutritious matter into the constitution, by putting in requisition the powers of the absorbents—by employing external friction with nutrient fluids; which, if any advantage could result from it, might be volatilized by appropriate combinations?

The suggestion is somewhat crude: thus much, however, may be said in its behalf; that if experiments fail of practical success, they at least have the effect of adding to our physiological knowledge.

To treat of the rationale of accomplishing these ends would involve some points of the relative pathology of the absorbent and producing powers, which, if my recollection serve me, have not as yet been adverted to; and, in order to elucidate our view of the matter, it will be necessary to recapitulate some particulars already noticed in the outline of the physiology of the absorbents.

Some Remarks on the Relation between the Absorbent and Secreting Systems, with their Application to Disease.

THE arterious and absorbent systems may be regarded as powers of antagonist functions. They have each an irritability particularly belonging to themselves, and they admit of distinct actions, whether temporary or permanent, exclusively belonging to either.

One of the effects of an increased excitement of the arteries is to produce an additional deposition of animal matter. The same end is accomplished by a diminished energy of the absorbents, while that of the arteries has in no wise digressed from the state of health. The effect of an increased excitement of the absorbents is to remove the depositions of secretion; and the same consequence results from a diminished excitement of the arteries, while that of the absorbents remains unchanged. Although this independence of affection must necessarily obtain, in a multiplicity of instances, the two systems

are nevertheless liable to partake of the same actions by the operation of stimuli, of which they are mutually susceptible.

It would appear, then, that diseases which arise from an increase of arterial excitement, may be removed either by a reduction of its excess or by an increase of the actions of the absorbent system, and *vice versa*.

In order to fulfil these indications, (which may be illustrated in the case of a common thickening of integument) we have recourse to topical bleedings, blisters, &c. upon the principle of diminishing the inflammatory diathesis which prevails in the arteries, and thereby restoring the balance which ought to subsist between them and the absorbents. Would it not appear, in this instance, that the same agents had a similar operation upon both? We treat inflammation of the absorbents upon general principles; we abstract blood, we employ lotions, and by these means we expect to reduce their excitement.

If this similarity of influence be granted, how can that be deemed a correct and legitimate principle which, on other occasions, dictates the employment of similar measures, with a view of obtaining a diversity of result? It will be replied, the actions of the arteries are morbidly increased, while those of the absorbents have undergone

no change; and it is with a reference to the condition of the former that such means are instituted.

If a susceptibility in common be in this case conceded, the force of the objection is in no wise lessened; and if this be denied, the principle must be inadmissible on other occasions. It sometimes appears (and it must be admitted to a certain extent, although it would be difficult to define all the instances) as if the mere state of morbid excitement were sufficient to determine the action of local applications to that order of vessels which is the most specifically affected by the disease. If such be the fact, it would be difficult to make out a rationale for its explanation; and it might be urged against it, that it is strenuously opposed by other principles, which must be regarded as almost indisputable. In order to elucidate these difficulties, we will take a closer view of the application of the means.

A person receives a contusion on the knee, in consequence of which blood is extravasated; inflammation succeeds; the violence of it subsides, and there remains only a slight degree of it, with a general thickening of the integument. The first inflammatory actions have been reduced by the employment of remedies, directed exclusively to that end.

In the progress of the case, another indication arises, viz. to produce the absorption of extraneous matter;

leeches are sometimes used for the accomplishment of this purpose, upon a principle no better defined than that of unloading vessels, and diminishing the local plethora which is supposed to perpetuate the condition of the part. I cannot conceive how absorption can be facilitated by any modus operandi implied in this explanation. It seems to include the idea of an obstruction of vessels from coagula, which the leeches are conjectured to remove. Their effect, however, can only be to abstract fluid blood from vessels which, cæteris paribus, will be filled in a ratio to the depletion they have undergone; the extravasated fluid being in general too deeply seated to fall within the sphere of their influence. The effect of their application may be more justly attributed to the diminished excitement which results from the abstraction of that blood, the presence of which becomes a concurring cause of the excess of arterial action.

Those who agree in this view of the matter conceive, that, by such measures, the absorbent function would be rendered more effective by balancing these antagonist powers. Blisters are employed for the same purpose, though directed by a somewhat different principle.

It has been assumed that these means would have the same effect upon the absorbent as upon the arterial system, reducing in common the actions of each. This point remains paradoxical only as long as we consider the operation of these agents with an eye only to a single

effect. It appears, from a general regard of all these considerations, that the following indications of treatment of the case we have supposed are the least objectionable.

Secretions and effusions are the consequences of increased vascular actions. The violence of these actions is diminished by the employment of the means, termed antiphlogistic. When the excitement is reduced to the standard of health, the natural relations of the secreting and absorbent powers are restored. But, in order to fulfil the indication, it is necessary that the energy of the latter should prevail, and exceed that of the former. Leeches and evaporating lotions are inadequate to this design; such means are best suited to it as effect the double purpose of restraining vascular excitement, and of increasing, at the same time, that of the agents of absorption.

In the whole catalogue of the means conformable with this view, there are, perhaps, none more decidedly efficient than blisters. They diminish the excess of vascular action upon the principle of counter-irritation; and although in this particular they may be supposed to have, on general principles, a similar effect upon all the parts of the structure; the absorbents are nevertheless stimulated to an increased activity by the immediate application of cantharides to their own surfaces, in consequence of the function which they specifically exert.

The benefit of some other means, as lotions containing the crude ammoniacal salt, vinegar, &c. may be explained in the same way: they reduce the temperature of the surface, and thereby diminish arterial action; while the stimulating properties which give effect to their use, increase the energy of the absorbents through the medium of absorption. The very frequent consequences (affecting particularly the urinary organs) which result from the employment of blisters, are a sufficient proof of the facility with which cantharides are absorbed.

These principles are of general application, and belong as properly to the case of enlarged lymphatic glands as to any aggregation of animal matter from a casual cause. Some cases, illustrative of the success of continued blistering, will perhaps form the subject of a future memoir. It would be superfluous to detail them here, while they may be briefly adverted to as facts which have, in my opinion, established the superiority of those means which have succeeded when others of higher reputation have failed.

My experience does not inform me whether a perpetual and alternate blistering of the abdomen and loins would, in the case of scrophulous enlargement of the mesenteric glands, be attended with the same happy result. The principle, however, is here admissible; the effect is to be determined only by the practice. It is probable that the means would fail, not because the design is

defective, but rather owing to the remoteness of the disease from the surface. It may be said, by way of suggestion, that the internal exhibition of remedies with the same view is not altogether precluded.

I shall hereafter consider, more particularly, the powers possessed by mercury, and what degree of merit it is entitled to as a means of facilitating absorption.

On Scirrhus and Fungus Hematodes as connected with the Absorbent System.

THE absorbents are liable to scirrhus thickenings of their coats, and in some instances they have assumed the actions peculiar to this tendency, and cancerous ulcerations have ensued. They may become affected with scirrhus either as an idiopathic disease, or in consequence of their contiguity to parts in which it is primarily established. The instances of the former are rare; and the difficulty of discrimination in both is, perhaps, a reason why the disease in them has not been more frequently and more accurately noticed.

As the absorbents are furnished with the same susceptibilities as other organized parts, it is but fair to presume that they are every where liable to participate in the diseases to which they are contignous. We should be warranted also in the supposition that they are sometimes the first order of parts to become affected

with diseases which are remotely propagated through the medium of their function. The minuteness of the examination, and the confusion incidental to parts which have undergone morbid changes of structure, must, however, preclude in general a demonstrative evidence of the fact.

But we are consoled in this instance for the insufficiency of our knowledge, by the assurance that we should derive little or no help in the treatment of such diseases by the most incontrovertible decision of the point.

It has been conjectured, by some respectable surgical authorities of the present day, that the absorbents subserve to the transmission of the matter of fungus hematodes, or soft cancer. The disease has been observed to extend in the course of absorption. It is probable that the absorbents themselves participate very largely in it.

By what means can the matter of this disease (supposing there to be any generated capable of producing it) or of cancer, become locally operative upon any other than the lymphatic parts of the structure? If the disease be propagated by absorption, the arterious ramifications can become affected by it only through the medium of those absorbents which help to compose them. How then can the absorbed matter produce disease locally upon any of the other constituents of an

artery, seeing that it is contained in absorbent tubes until it becomes mixed in the general circulation? It appears necessary to adopt one of two conclusions; that the diseases in question are either not propagated by absorption at all; or, if they are extended in this way, that the absorbents themselves are primitively and specifically affected by them.

Dropsical Affections, as dependent upon the State of the Absorbent System.

ANASARCA, or an accumulation of fluid in the cellular substance, may result either from an increased excitement of the arteries, or from a diminished activity of the absorbents; or from vascular obstruction, impeding the return of blood to the heart.

Perhaps this affection in general may be attributed with more propriety to a modified action of the secerning, rather than to any morbid peculiarity of the absorbent, function. The two powers are, however, so strictly interwoven and complicated with each other, that we can scarcely explain the consequences which may result from a derangement of the one, without including in our regards the properties and offices belonging to the other; and this will, perhaps, form a sufficient apology for adverting to a disease which cannot be said, under all its forms, to belong exclusively to those agents, some of the phenomena of which I have undertaken to consider.

We find all the varieties of dropsical affection sometimes associated with an unusual vigor in the actions of the heart and arteries. Under the existence of this truly inflammatory diathesis we should, I think, be warranted in the inference that the accumulation of fluid in the cavities of the body resulted from the excess of vascular action.* The supposition is also supported by collateral evidence.

In examining the state of the arteries in persons who have died anasarcous, I have myself observed the internal coat to bear generally the most unequivocal marks of inflammation; an inflammation, which in one instance appeared to have induced diffused spots of ulceration of the vessel at irregular distances, and to have deranged materially its general structure and appearance.

The fluids contained in the absorbents (which are found to resemble those in the cavities from whence they are absorbed) in a state of health coagulate on exposure to atmospheric air; while those effused and absorbed in consequence of a dropsical tendency, are sometimes incapable of coagulation. The coagulating propensity is, however, most unusually strong and decided where the effusion is produced by a manifestly inflammatory process.

* This has been particularly noticed by Mr. Freer, of Birmingham, in his observations upon aneurism and some diseases of the arteries. He also cites some cases decidedly illustrative of the benefits resulting from an employment of the means suggested by the indication.

These facts form a more than presumptive evidence that the disease proceeds from a derangement in the secretory functions. They do not, however, amount to an absolute proof, since it is not impossible but the fluids may undergo some chemical changes from the unusual time they are reposed in their cavities. Some particular influence may also be produced upon them by a modified exertion of the absorbent function. These objections, however, are not of sufficient force to invalidate the preceding arguments.

On the other hand, we frequently see dropsy succeeding to profuse hemorrhages, and associated with the most unequivocal marks of debility. How are we to reconcile this identity of effect with causes of so reversed a tendency? The same state of debility, and the same consequences flowing from it, frequently result from the exhaustion consequent upon febrile diseases.

It appears to be a law in the animal economy, that the muscular fibres are constricted and compacted, to a certain extent, in a ratio to the degree of that nervous excitement to the influence of which they are so materially subjected. The varieties of this excitement are found in its deficiencies and its excess; and these states are otherwise expressed by the terms debility, and a strength exceeding that by which health is denoted. It is easy to conceive that the effect of the latter, under some circumstances, may be to increase the quantity of the secretions by the additional impetus which is thus

conferred upon the fluids in general. A state of the secerning organs different in its nature but similar in its effect, may be discovered in the enlargment of the caliber of the secretory tubes. This enlargement of caliber might ensue from the distention they have undergone during an inflammatory diathesis, the subsidence of which has been followed by a state of general debility. It happens, however, very commonly, that subsequent to such febrile attack the actions of the body are renewed; and the excitement, at first reduced, is gradually restored to its pristine degree. In that case, the condition of health becomes re-established. While we observe the effects of inflammatory actions upon the powers of secretion; if we turn our regards to the condition of the absorbents, we shall find that the same cause has a concurrent operation upon both functions for the production of hydropical disease. The absorbents appear to participate very decidedly in all febrile actions: indeed, the balance which ought to subsist between absorption and deposition is in this instance destroyed, and the former process so considerably preponderates, as in a comparatively short period to produce an almost incredible reduction of the original bulk of the body.

Consequences, analogous in some respects, will result from an increased energy and activity of the absorbent, as of the arterious, systems. The excess of excitement will in them be succeeded by a proportionate debility, and the same collateral causes will mutually prevent a recovery of their powers. The absorbents, then, are

thus incapacitated from removing that accumulation of fluid which is secreted with morbid profusion by the antagonist function; incapacitated from the operation of a cause affecting both sets of powers in common, though producing in each a modified result.

Hemorrhages lead to dropsical affection, by inducing that state which has been termed a relaxation of the solids. The loss of blood, on these occasions, produces a reduction of excitement inadequate to that constriction of the muscular fibres which obtains when the vessels are said to possess a proper and sufficient tone. Hence an increase of caliber, tending to the same effect as when the secretory functions are accelerated by an inflammatory diathesis prevailing in the arterious system.

Another cause of the accumulation of fluids in the cavities of the body is vascular obstructions, produced either by changes of structure and a partial or total obliteration of vessels, or by the pressure of growths either morbid or natural, not belonging to the original condition of the animal.

Instances of this kind are met with in the case of ascites produced by a diseased structure of the liver, &c. and also in the œdema affecting the lower extremities in consequence of the pressure which the gravid uterus exerts upon the ilial veins. Watery accumulations, produced in this way, do not form strictly a subject of our present considerations: they may, however, be ad-

verted to as diseases, for the cure of which the absorbents may occasionally be usefully employed; and so far a remote alliance subsists between them.

For the treatment of dropsical affections, then, the following indications appear to arise from the principles it has been attempted to establish. They sometimes result from an increased action of the arterious system. This cause is discriminated by the state of the pulse, and other marks by which inflammatory actions are generally characterised. Here, the means of reducing this excess of action should first be employed. Among these means are emetics, purgatives of the drastic kind, together with all such agents as promote the excretions, and more particularly that of the urine.* Digitalis and depletion by the lancet would also appear to be comprehended in the indication. The latter is objectionable by reason of the debility which may succeed to its employment, leading to the effect it was designed either to remedy or avert. How far the practice would be safe and beneficial under the circumstances of a marked excess of excitement, my experience does not enable me to determine. I have seen it employed when its use was not

* The use of drastic purgatives has been of late much reprobated. They are objected to on the score of their stimulating properties. This objection, however, appears unfounded; because medicines of this kind are not taken up from the intestines in any efficient quantity and conveyed into the system: their immediate operation, on the contrary, is limited to the bowels; and they produce, by this action, an effect on the constitution, quite the reverse of stimulation.

very decidedly called for, and the finale of the disease appeared to be hastened by its adoption.

When dropsy is associated with evident debility, manifested more especially in the actions of the heart and arteries, the indication, as it regards the powers of secretion, becomes reversed. General measures of stimulation fail, however, because they produce only a temporary increase of excitement, without conferring any addition to that irritability which is so essential for the contraction of a muscular fibre. It appears, in these complaints, an universal indication to increase the activity of the absorbent system.

General remedies are inapplicable with this view when an excess of vascular action obtains, as the means employed would have the effect of raising it to a still higher degree. There are some agents which have obtained a popular credit for possessing the power of accelerating exclusively the absorbent function. Among others of this kind, vinegar is honored with the highest reputation.

Upon a principle, suggested by Fourcroy, it is not impossible but some remedies of this kind may be discovered; and there would be no objection to their trial, since they are so directly referred to by the indication.

Blisters, in some of the forms of dropsical disease, are occasionally followed with the most beneficial results,

and their use would sometimes be indicated with additional force by the modus operandi which has been assigned them.

It appears of the first importance in all the varieties of dropsy, to increase by every possible means the quantity of *all* the natural excretions. A derivation of fluids is thus produced from the seat of the disease; and the operation of such means would, in some instances, be rendered doubly advantageous by their additional tendency to reduce arterious excitement.

It is superfluous to make a formal and elaborate detail of all the species of hydropical affection; and it is equally unnecessary to enumerate the means so well known, for the fulfilment of the several indications.

Inflammation of the Absorbents.

AN instance of the morbid irritability of the absorbents, exclusive of any participation of the arterial system, occurs in the state of inflammation peculiarly affecting themselves. The state appears to *commence* with this independence of affection, although in the progress of the disease it becomes propagated and extended to contiguous parts. Distinct lines of inflammation are visible, running from the place of the injury to the absorbent glands, which soon assume the same actions: the subject of the affection also expresses great pain, particularly on pressure, before the process of tumefaction has commenced. And when swelling has supervened, it appears to arise from the sympathy subsisting between adjoining structures.

It would admit an easy explanation if this state of the absorbents were produced exclusively by the absorption either of badly formed matter, as in the case of an ill-conditioned stump, or by the infliction of wounds

upon which an absorption of a virus may be supposed to have supervened. This, however, is not the case. Inflammation of the absorbents frequently succeeds to injuries, in which the remotest suspicion of the absorption of matter capable of generating disease cannot possibly be indulged. They sometimes undergo a spontaneous attack of inflammation without any external assignable influence.

Those wounds which are denominated punctured, frequently produce it; and the friction of a stirrup, saddle, &c. without even the slightest abrasion of the skin, is a very common cause of its occurrence.

In some erysipelatous inflammations the absorbents partake very decidedly in the actions which are thus established: the lymphatic glands of the groin, axilla, &c. swell up, the disease extends in the course of the absorbents, and all the phenomena afford the most unequivocal evidence of their participation.

It is highly probable, although the absorbents are interested in almost all the species of phlegmasiæ, that the primary affections of them in such instances are very limited. Diseases of this kind are always accompanied by a greater or lesser degree of tumefaction.

From other pathological principles, and more especially from considering the phenomena of an inflammatory diathesis affecting the arteries, we have reason to

believe that a *diminution*, rather than an increase of bulk, would ensue from simple inflammation of the absorbents. It is probable that in the majority of instances the other orders of vascularity first assume an excess of action, that the absorbents partake of the same, and that this state of the latter is associated only with a similar condition of the former.

The consequences of many phlegmonous diseases are not confined merely to an indurated enlargement of bulk: they very commonly lead to effusions; to the formation of those vesicles by which erysipelas is characterised, and ultimately to a state of ædema prevailing throughout the whole extent of the disease.

If these effusions supervened only upon the subsidence of the attack, the circumstance may be explained upon the principle of a diminished absorbent function, resulting from the debility which succeeds to an excess of excitement. But they occur in all the periods and stages of the disease, and appear the most abundant when the diathesis is the most violent.

This is precisely the reversed effect of an exclusive inflammation of the absorbents. It is easy to conceive that the absorbent function may remain locally impeded, when the tendency to increased action has been completely subdued. And this is more particularly liable to happen where matter of morbid production has been generated, and has been specifically operative upon the

absorbents, through the medium of that function which it is their business to exert.* These considerations are sufficient to explain all the phenomena of phlegmonous inflammation, as far as the absorbents are interested in the process; and they apply very extensively to this class of diseases. Although it must be conceded that there are some few exceptions; it must, on the other hand, be allowed that the absorbents themselves, in the varieties of phlegmasiæ, are very rarely the first to assume an inflammatory action. Inflammation of the absorbents terminates commonly when it leads to organic disease, by inducing either suppuration, or a modified structure of their tubes, arising from the effusion of coagulating lymph.

The actions of the vessels of the absorbents are sometimes so changed as to render them capable of secreting calcareous matter. This result is occasionally manifested by the discovery of calculi contained within the absorbents, more frequently in the glands belonging to them than in the lymphatics themselves. There are other diseases of structure which may be induced in persons of peculiar idiosyncracies by a primary state of inflammation of the absorbents. But as many of such as can be conveniently admitted are noticed elsewhere.

* The consequences of such phlegmonous inflammation may be advantageously treated when its actions have entirely subsided, by exciting the activity of the absorbents by means which will be subsequently noticed.

Inflammation of the absorbents sometimes leads to an obliteration of their cavities by an effusion of coagulable lymph. This effect is either more common, or else more evident, in the lacteals than in any other orders of the absorbent system; and the glands are more liable to such imperviousness than the lacteal tubes themselves, although in them it is frequently complicated with the processes of some specific disease.

Obstructions of this kind are in part remedied by the same natural provisions as those with which the vascular system in general is furnished. In the case of an obstructed lacteal gland, the fluid which would otherwise pass through it, is partially transmitted by lateral anastamoses opening into the lacteals above the obstruction. These lateral anastamoses are not limited to that order of absorbents called the lacteals; the same provision for averting the effects of diseased actions is instituted throughout the lymphatic system.

The absorbent anastamoses are much more numerous than those either of the arteries or the veins. It is probable that a communication subsists between the lacteal and lymphatic vessels of the mesentery; it would otherwise be difficult to explain the manner of nutrition, imperfect as it is, which takes place in tabes mesenterica, under which disease the lacteal system sometimes appears entirely incapacitated for its office.

The thoracic duct itself has been found totally obstructed by calcareous matter. It is not difficult to conceive that this might arise from a primarily increased action of the vessels of the tube, leading, in certain individuals of peculiar habit, to that morbid secretion which constitutes the effect. In this instance, also, the business of nutrition still goes on; and that too in a case related by Mr. Cheston, of Gloucester, with every appearance of a function but slightly impeded.

Sometimes the thoracic duct is ruptured, and its contents are effused into the cavity of the chest: this is probably the consequence of a local inflammation leading to an ulceration of its coats.

In Mr. Cheston's case, the matter of nutrition must have been conveyed into the sanguiferous system by anastamoses communicating with the main trunk both above and below the obstruction.

All these instances go to prove (what indeed from other irrefragable evidence cannot be doubted) that the absorbents possess an irritability peculiarly belonging to themselves, and that they admit an excitement exclusive of any other vascular participation. Without this concession, it would be impossible to explain many of those phenomena of absorption which allow an easy solution upon this principle.

The case of inflammation of the absorbents which the surgeon is the most frequently called upon to attend, is of casual occurrence, and is to be treated by the antiphlogistic means, under the employment of which it commonly subsides, without leaving any of those tendencies to subsequent disease, which may result either from a change of structure, or a materially impeded function.

Some of the principles of the pathology of inflammation of the absorbents, are here only cursorily adverted to: the brevity necessary to be observed where so many particulars are to be noticed, precludes a more extended discussion of the rules, practices, and observations, elsewhere sufficiently defined.

On some Diseases of the Bones as connected with the Absorbent System.

MOLLITIES ossium is a disease of comparatively rare occurrence. We have here another instance of the reciprocation of function, tending to morbid results, subsisting between the processes of absorbtion and secretion.

Mollities ossium is a condition of the bones in which they become preternaturally soft and flexible. This state of them may be produced by derangement either of the absorbent or the secreting systems; the one tending to a *removal* of the phosphate of lime which gives them their firmness and solidity, and the other not subserving to the purpose of its production. It is probable that both these causes may concur in the same end.

In the majority of the cases on record of this disease, a calcareous deposition has been found in the urine, upon

evaporation, somewhat analagous to mortar. While we regard this as a proof of a deranged absorbent function, it does not altogether invalidate the conjecture that the process of secretion is also more or less implicated as a cause. It is no criterion, by which a discrimination may be formed, between the affections of these antagonist powers.

A case is related by Mr. Gooch, in which this preternatural softness of the bones was preceded by its reverse, an unusual brittleness; subjecting them to fracture when only the weight of the body was reposed upon them. In the detail of the case the state of the urine is scarcely noticed. It is probable, from some considerations, that it was exceedingly scanty; and if marked by any excessive deposition of calcarous matter, he could hardly fail to have insisted particularly upon the circumstance. In this instance, I should suppose that the processes of secretion were the most specifically concerned: we can nevertheless hardly conceive that they could become so suddenly and so completely reversed. We are informed, in the dissection, that the knife passed through the bones without resistance. The disease began in the centre and extended outwards; and the periosteum did not appear to have had any share in its production.

If this state of flexibility had been produced by inordinate absorption, we must suppose that the absorbents first removed the more fluid parts of the bone, together

with that uniting medium which serves to strengthen the cohesion of the calcareous particles. The process was next exerted upon the phospahte of lime, the removal of which would constitute the state we are considering. The peculiarity of the absorbent function necessary for this end, appears to consist in no more than an inordinate activity.

The question then arises, why did the absorbents destroy and remove the firm, dense structure of the bones, and allow that parenchymatous, liver-like substance to remain, with which their cylinders were ultimately filled?

The force of this objection is not easially obviated, unless we attribute to the absorbents a function governed by an elective attraction, which affects different affinities under the modifications of disease. This would be to admit an agency which cannot derive sanction from any evidence, either collateral or direct. The possibility of the thing has also been elsewhere shewn to be extremely dubious on other accounts. It is worthy of remark, however, that mollities ossium was in this case associated with anasarca. A case is also mentioned by Mr. Thompson, in the Medical Observations and Enquiries, in which calculi were discovered in the urine.

Mr. Latta details another, in which there occurred a most copious deposition of earthy matter from the urine, which was always followed by an alleviation of pain.

From a survey of the facts relative to this disease, which may be gathered from many quarters, I should be inclined to adopt that explanation in which a deranged secretory process was regarded as principally operative. A modified function, in this respect, may lead to an imperfect formation of the constituents of bone. It might have produced, in the case related by Mr. Gooch, first fragilitas ossium, by a suspended secretion of the medium of union between the calcareous particles. The same office being longer withheld, would lead to that decomposition of the bone which must ensue from an inadequate cohesion of its parts. While the function in this respect was suspended, the production of calcareous matter might have gone on unimpeded; in addition to which, it is but fair to presume that some other secretions took place, in consequence of the morbid actions which must necessarily have prevailed. This calcareous matter, no longer united and compacted with that gelatine and cartilage by which it is usually consolidated, would admit an easy absorption, without supposing any great degree of derangement of this function, and hence the peculiarities by which the urine was characterised. There is, however, so much obscurity in the pathology of this disease, that it would be presumptive in the extreme to hazard a suggestion about the rationale of its treatment.

It is necessary to suppose a *modified* action, whichever set of powers we conceive to be principally concerned in its establishment; and this term, modified, in-

cludes some pathologieal peculiarities of a more difficult comprehension than is implied by either of the terms, inereased or diminished exeitement. It most probably involves some latent and, perhaps, inexplicable change of structure, upon which the peculiarity of the actions might depend.

If any mode of treatment were indicated by the premises, it would, perhaps, be the copious exhibition of animal glue. Where the pathology is so intricate, that the prineiples which relate to it are inadequate for its developement, to discover a remedy is rather the business of empirieism than of the exereise of reason.

That tendency whieh sometimes manifests itself in the bones to form exostoses, seems to reflect additional credit upon the supposed propensity to a modified funetion of the secerning system belonging to the structure of a bone.

This tendency sometimes amounts to such an excess, that in whatever part of the body the subjeet of it receives a blow, however slight, an osseous tumor is the consequence. It has been supposed to proceed from a superabundance of phosphate of lime, in consequence of an impeded absorption. Experiments, and a treatment eorrespondent with this view, have been instituted, at first, with apparent suecess; but, unfortunately, there were some clashings of results between the first and the subsequent instances.

These results were, in short, quite contradictory; and the projectors, ashamed of the offspring they had cherished, abandoned their theory in despair; and it sunk, past redemption, to the lowest depths of the waters of oblivion.

The disease may possibly arise from a constitutional and morbid irritability of the periosteum; disposing it to inflame from the operation of slight stimuli, and effusing osseous matter in consequence. The fate of the theory hinted at, affords no encouragement to say another word in support of this conjecture.

On some of the Means of exciting the Actions of the Absorbents.

IT remains now only to bestow a few considerations on the general and local means of increasing the activity of the absorbent function. Among the former, mercury is one of the most approved remedies.* Its operation is most decidedly beneficial in some cases of effusions into the cavities of the brain. These effusions might proceed either from the actions which ensue from casual injury, or from the same actions established spontaneously by a constitutional cause.

* It is worthy of remark, that the susceptibility of being affected by mercury, is greatly dependent upon climate, and perhaps upon peculiarities of the absorbents induced by the same cause. In the Lascar, the thoracic duct is twice as large as in the European; and such men will bear the introduction of unusual quantities of mercury before the constitutional operation of it becomes manifest. It is probable that in them the secretions may be increased by the great elevation of temperature to which they are subjected: this increase of secretion demands a proportionate activity of the absorbents, and these vessels lose a great portion of their irritability by such excess of action; and therefore are not so readily excited to exert their function by ordinary stimuli.

We have here another instance of that independence of irritabilities which has been before so particularly insisted upon; for although mercury be a stimulus, not only to the absorbent, but likewise to the arterial system, we must nevertheless conclude that it affects the former in the greatest degree. From this consideration we are to infer, that the means of diminishing secretion should precede the exhibition of this remedy: previous to the accomplishment of this end, it may be supposed a priori, that the balance of irritability prevails in favor of the agents of secretion; and the effect of introducing into the system a stimulus which is alike operative upon both sets of powers, would be to cause that to predominate in which the greatest share of irritability obtained: thus *inducing* the condition it was designed to obviate. The vascular actions should first be reduced by leeches, depletion from the temporal artery, and blistering the scalp: in the mean time, those means which promote the excretions, would in some degree counterpoise the tendency of the local measures, to diminish, in common, the activity of the absorbents.

When the progress of secretion is restrained, or its disposition to excess altogether subdued, the use of mercury may be resorted to with advantage, as a powerful means of producing the absorption of that fluid which was previously effused.

It has been already noticed, that the absorbents are the most immediately excited to increased actions by

those agents which obtain a primary operation upon them, by being conveyed into the system through their intervention.

Among these means, blisters, under judicious restrictions, claim our first regards. Ointments, containing small quantities of tartarised antimony, are also highly efficient on some occasions, upon the same principle.

Camphorated mercurial ointment comes under the same description: but, as far as my experience has gone, its powers in this way are not so decided as those of the means before mentioned.

We have reason to believe that the majority of morbid growths are furnished with absorbents in common with the other orders of organization. Some of these are of too malignant and dangerous a character to fall properly under the indication. It appears, however, that the employment of the absorbent function may sometimes be advantageously called in, for the removal of certain incipient tumors, or even of those which have attained some bulk, where the more speedy assistance of the knife is rendered inadmissible by anatomical considerations. We have reason to suppose, from analogy, that tumors of the steatomatous kind, and some others, may be advantageously treated by perpetual blistering, or by the alternate use of blisters and ointments containing tartarised antimony. A reduction of the arterious

excitement of the part by leeches should first be premised, and repeated as circumstances may require. The principle is not limited to this kind of tumor; but may be extended to all such as would admit the application, without the hazard of assuming malignant actions under its discipline.

Pressure is a means of promoting absorption which frequently merits the highest regard. Its effect, however, is not strictly limited to an agency upon the absorbents. While it promotes the energy of this function, in some of the forms of its applications, it leads to actions in the other vascular structures which are sometimes followed by an effusion of coagulating lymph, producing an increase of bulk at places a little remote from the point of its immediate action.

This objection, however, is only applicable to such circumscribed pressure as is occasionally exerted by the tourniquet, &c. and does not obtain, at any rate so manifestly, where the compression is more diffused, as by the application of bandages. At best the objection is an inconsiderable one; although instances are not wanting in which such effect is extremely palpable.

Circumscribed pressure sometimes produces the entire absorption of the integument immediately under it, and the body which exerts this pressure becomes adapted to the cavity which it has thus produced. From what has been before observed, it appears difficult

to conceive that this should be exclusively the effect of the absorbents; I should rather suppose that the pressure prevented the entrance of blood into the vessels, and that the vitality of the part became locally extinct: in the course of this process the structure would be broken down, and thus fitted for the operation of the absorbent function.

Electricity is likewise employed for the purpose of inducing absorption. My own observations will not allow me to feel very sanguine in the anticipation of much good from its use. I have had many opportunities of noticing its operation: but its action has always appeared so transient, that in no single instance can I remember its leading to any permanent benefit.

Friction is another means of exciting the activity of the absorbents: it is admissible only when the local irritability of the arterious system has been previously completely subdued. If employed when these premises have been omitted, it produces inflammation and increased effusions; and a disease, otherwise dormant and tending daily to a complete subsidence, is excited to reiterate its actions, and the establishment of it becomes, perhaps, more irremediable than at any previous period of its existence.

On the subject of the functions and pathology of the absorbents, a great deal remains to be discovered.

If any of the hints I have hazarded, should at any time lead to a successful practice, or even to a more profitable discussion, I shall, in that exclusively, have obtained a more than ample reward.

The age is not so wholly forsaken by the genius of inquiry, but that we may expect, in the progress of the investigation, a more abundant harvest from those who enjoy better opportunities and higher qualifications for research.

FINIS.

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